tures up to 60° C., is described and figured at p. 348, and consists essentially of a closed vessel with triple walls, the space between the inner and middle plate filled with water, the outer containing air. For higher temperatures a simple tin plate thermostat was employed, the space being filled with water for temperatures up to 100° Cent. and with glycerine or oil for higher temperatures. The source of heat was always a gas-flame with the usual thermo-regulator. Numerous tabulated results are given of experiments upon moist and dry seeds at various temperatures, and it was found, as might be anticipated, that perfectly dry seeds can withstand a high temperature, even between 120° and 125° Cent., without injury.

Dr. Koch describes how bacteria can be observed, prepared, and photographed, this paper forming the sixth of the extremely important series of researches on bacteria which have from time to time appeared in the *Beiträge*. A thin layer of bacteria with the fluid containing them is to be dried on a thin cover of glass. By placing the glass cover with the dried material in absolute alcohol, or better, in a 0·5 per cent. solution of chromic acid, the bacteria are fixed to the cover, although the coagulated ground substance in which the bacteria are imbedded can be made to swell up and the bacteria themselves to resume their natural forms when the cover is placed in a solution of acetate of potash (1 part to 2 of distilled water). The bacteria can be coloured by means of aniline, the best of all being aniline brown; but methyl violet and fuchsin will also answer. The stained object can be preserved permanently on slides by mounting in Canada balsam, concentrated solution of acetate of potash, or in glycerine. Twenty-four photographs of bacteria, mostly from specimens stained with aniline brown, illustrate the paper; and in some, as 5 and 6 on Plate XIV., the cilia of bacillus are very beautifully shown, magnified 500 and 700 diameters. Koch finds that it is easier to photograph the cilia than to observe them directly with the microscope.

The other papers in this part are on certain Ustilaginæ, by Dr. Schroeter; and on two new species of Entomophthora (*E. conglomerata* and *E. rimosa*) discovered upon dead gnats, by Prof. N. Sorokin.

The first and second parts of vol. iii. contain eleven papers. Four of these are devoted to Bacteria, and form the seventh to the tenth of the series of Researches on Bacteria already alluded to. The titles of the papers are VII. Experiments on Infection with *Micrococcus prodigiosus*, by Dr. A. Wernich; VIII. Researches on the Bacteria in Air, by Dr. Miflet; IX. On the Action of the Electrical Current on the Multiplication of Bacteria, by Dr. F. Cohn and Dr. Mendelssohn; and X. Studies of Blue Milk, by Dr. F. Neelsen. Two of these papers may be briefly mentioned. By means of a specially contrived apparatus fitted with a new continuous aspirator, the invention of Paul Boehme in Brunn, atmospheric air from different localities was examined. These were (1) air in Botanical Laboratory; (2) in Fever Hospital; (3) in the Pathological Theatre; (4) in the Surgical Theatre; (5) air in Botanic Garden; (6) air for soil; and (7) air for drains. The results were briefly as follows:—1. Germs of bacteria capable of developing are abundant in the air, and could readily be collected and cultivated in a special mineral solution, malt extract, or solution of Liebig's extract of beef. 2. Many forms of bacteria can produce reproductive germs in air, while others, as *B. Termo*, seem only capable of producing germs in putrescent matter. 3. Air from the soil contained occasionally germs of bacteria. 4. Air from the Fever Hospital contained no germs, owing to the completeness of the ventilation and disinfection. 5. Air from a sewer contained abundance of germs of bacteria capable of reproducing.

Neelsen, in his paper on Blue Milk, finds that the special organism in it may assume three or four different forms, sometimes like Bacterium, then like Bacillus, then like a Chroococcus, and lastly like a Leptothrix. He discusses the Theory of Cohn and others that the Bacteria form many separate genera and species, and the Theory of Lankester and Warming, that they are forms of a protean species, and seems to conclude that the germs of a given form may under different conditions develop in one or other direction, as observed by him in blue milk.

Dr. Schroeter continues his observations on the Development of Rust, and Dr. Oscar Kirchner describes the Development of *Volvox minor*, Stein. Dr. Hielsher describes the Anatomy and Biology of the Genus Streptocarpus, and details many interesting facts regarding that curious and beautiful genus. When the seed of *Streptocarpus polyanthus* germinates, numerous adventitious roots form on the primary axis, one of the two cotyledons soon disappears, while the other develops greatly, and forms a perennial foliage leaf. On the petiole of this leaf numerous adventitious roots develop and the primary axis disappears. The leaf produces adventitious buds from which the flowers develop, while it also develops a series of adventitious leaf-buds. Dr. Beinling contributes a paper on the formation of adventitious roots and buds on the leaf-cuttings of Peperomia. Prof. Klein describes in detail the anatomy of *Pinguicula alpina* as an insectivorous plant, and points out that the plant occurs in two forms, one with green leaves, the other with the leaves more or less red-brown in colour, and that the tissues assume an intense yellow colour when acted on with caustic potash solution. The remaining papers are by Dr. Schwartz, Chemico-botanical Studies on the Acids in Lichens, and Dr. Eidam on the Gymnoasci. The various papers ably sustain the reputation of this work, and all of them will well repay careful study.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Dust and Fogs

I MUCH regret the Hon. R. Russell, in his letter to NATURE, vol. xxiii. p. 267, takes such an extremely desponding view of the influence which my experiments on cloudy condensation are likely to exercise upon the present attempts to rid the atmosphere of our large towns of their ever-recurring fogs. The object of these experiments was to find out what caused fogs, in the hope that with the knowledge thus acquired we might be better able to find a remedy. The preferable course seemed to be to find the cause first, and then if possible devise some remedy, rather than try remedies at haphazard.

It is certainly very far from my desire to discourage the present attempts which are being made to clear the atmosphere of our large towns of smoke, and I have recognised the advantages which would result from the adoption of more perfect forms of combustion. In my paper I have simply distinguished between fogs and smoke, and separated them for distinct consideration and treatment, and have at the same time directed attention to some points which ought to be considered before deciding on their prevention.

With regard to Mr. Russell's difficulty in reconciling the result of the experiments with what is observed with regard to fogs in London, Paris, and other large towns, it appears to me to have arisen entirely from not putting sufficient weight on the all-important influence of the amount of vapour in the air of the different places. It is condensed vapour which forms the fog, and dust simply determines whether it will condense in fine- or coarse-grained particles. The atmosphere of Paris, compared with that of London, is an extremely dry one, and the air is seldom in a condition to produce fogs. The atmospheres of the other towns mentioned are also drier, some of them very much drier, than that of London. London however will probably be always more subject to fogs than other cities on account of its great size, some part of it being always in its own smoke.

Considered from a different point of view, might not the fog of January 31, 1880, referred to by your correspondent, be cited in evidence of a conclusion the opposite of that drawn by the writer, and in favour of the correctness of the experimental results? From this point of view the low white fog cleared away because it was formed in the comparatively pure air of the streets, while the higher fog did not clear away because it was formed in the products of combustion The true explanation however would rather appear to be, that where the fog was white it was also of less depth than in those places where it "extended high" and mixed with the smoke; and the sun, which was only sufficient to dispel the lesser depth "more or less," would evidently be insufficient to clear away the greater depth. It is however impossible to form any definite idea as to how this particular fog conducted itself, without much fuller information as to air-current, &c.

I have communicated to the secretary of the Royal Society of Edinburgh a second experimental paper on fogs, with special reference to dry fogs. In this paper the full answer to the latter part of Mr. Russell's letter will be found. JOHN AITKEN

Darroch, Falkirk, January 24

Professors Exner and Young

MY statement in respect to Prof. Exner's having announced the thermo-electric neutrality of a bismuth-antimony pair immersed in pure nitrogen, rested upon a note in NATURE (vol. xxii. p. 156), and this it seems was based upon a statement in *L'Electricité*. I have seen those of Prof. Exner's papers which have appeared in the *Annalen der Physik*, and there is certainly nothing of the sort in *them;* but I supposed that it must be contained in some other paper in some one of the numerous other publications to which I have not access here. It never occurred to me, until within a very short time, that there could be any mistake as to his having made such an assertion. How or where the error originated I cannot quite understand; but I trust Prof. Exner will accept my apologies for my share in its propagation, and that he and all concerned will be satisfied that no misrepresentation was intended on my part. The incident is a good illustration of the extreme care necessary in commenting upon the views of another person. C. A. YOUNG

Princeton, U.S.A., January 12

The Flying-fish

IT is remarkable that there should still be any doubt as to the facts in connection with the flight of the flying-fish. Dr. Günther ("Study of Fishes," p. 622), summarising the observation of Möbius, says that "they frequently overtop each wave, being carried over it by the pressure of the disturbed air" (in the open sea!). Again, flying-fishes "never" fall on board vessels "during a calm or from the lee side." At night "when they are unable to see they frequently fly against the weather-board, when they are caught by the current of air and carried upwards to a height of twenty feet above the surface of the water." Surely the fish going at the rate of at least ten miles an hour would on striking the "weather-board" be dashed, bruised and helpless, back into the water instead of coming over the side fresh and vigorous, flapping about on the deck. Except when "by a stroke of its tail" it turns towards the right or left, Möbius concludes that "any deflection from a straight course is due to external circumstances, and not to voluntary action on the part of the fish."

I have watched flying-fish repeatedly, and have invariably seen them fly, or rather glide, over the surface of the sea, and from one to two feet above it, rising gently to the swell when there was no wind, and occasionally turning to the right or left without touching the water. I do not say that when there is a breeze the tail of the fish may not touch it, but I think that, with the foam and spray of the broken water, it would be very difficult to be sure of it, and, moreover, if the tail was used the motion would be a jerking one. Mr. Wallace speaks of their "rising and falling in the most graceful manner," which, although he is referring to another species, applies also to the North Atlantic form (*Exocœtus evolans*). Mr. Bennett ("Gatherings," &c., p. 14) says that they "spring from the sea to a great elevation." This is probably in reference to their coming on board ship at night, attracted, it is supposed, by the lights. I believe the pectoral fins are kept extended without any motion, except perhaps as Mr. Whitman,[1] a recent observer, says, just when they rise from the sea. He gives 800 to 1200 feet as the greatest distance he has seen them fly, and about forty seconds as the longest time out of the water. By what mechanical means they move when out of the water is still to me a mystery.

I have never known the flying-fish to be pursued by other fish, nor ever seen any bird near them; indeed few birds are ever seen far from the land north of the southern tropic, where flying-fish are most abundant. The dolphin (*Coryphæna*) is supposed to be their greatest enemy. I had once an opportunity of seeing one opened—in the West Indies—its stomach was quite full of *Orthagoriscus mola*, very young, being not quite an inch long.

FRANCIS P. PASCOE

1, Burlington Road, W., January 21

Mr. S. Butler's "Unconscious Memory"

I MUST reply to the review of my book, "Unconscious Memory," in your issue of the 27th inst., and to Dr. Krause's letter on the same subject in the same issue.

Mr. Romanes accuses me of having made "a vile and abusive attack upon the personal character of a man in the position of Mr. Darwin," which I suppose is Mr. Romanes' way of saying that I have made a vile and abusive personal attack on Mr. Darwin himself. It is true I have attacked Mr. Darwin, but Mr. Romanes has done nothing to show that I was not warranted in doing so. I said that Mr. Darwin's most important predecessors as writers upon evolution were Buffon, Dr. Erasmus Darwin, Lamarck, and the author of the "Vestiges of Creation." In the first edition of the "Origin of Species" Mr. Darwin did not allude to Buffon nor to Dr. Erasmus Darwin, he hardly mentioned Lamarck, and he ignored the author of the "Vestiges" except in one sentence. This sentence was so gross a misrepresentation that it was expunged—silently—in later editions. Mr. Romanes does not and cannot deny any part of this.

I said Mr. Darwin tacitly claimed to be the originator of the theory of evolution, which he so mixed up with the theory of "Natural Selection" as to mislead his readers. Mr. Romanes will not gainsay this. Here is the opening sentence of the "Origin of Species":—

"When on board H.M.S. *Beagle* as naturalist, I was much struck with certain facts in the distribution of the inhabitants of South America, and in the geological relations of the present to the past inhabitants of that continent. These facts, as will be seen in the latter chapters of this volume, seemed to throw some light on the origin of species; that mystery of mysteries, as it has been termed by one of our greatest philosophers. On my return home it occurred to me in 1837 that something might perhaps be made out on this question by patiently accumulating and reflecting upon all sorts of facts which could possibly have any bearing on it. After five years' work I allowed myself to speculate upon the subject, and drew up some short notes; these I enlarged in 1844 into a sketch of the conclusions which then seemed to me probable; from that period to the present day I have steadily pursued the same object. I hope that I may be

[1] See *Zoologist* for November, 1880.